

ANALYSIS

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SOME REMARKS ON EMPIRICISM

By CARL G. HEMPEL

IN this note, I want to deal with the objections which Dr. v. Juhos has raised in this periodical¹ to certain considerations concerning the language of science, which are mainly due to Carnap, Neurath and Popper, and some of which I have outlined in an earlier paper.²

We may distinguish three main points to which v. Juhos's criticism refers: (I) The "behavioristic" interpretation of psychological statements such as "I see blue"; (II) the view that in our empirical science even statements of the kind just mentioned might be "altered" or "abandoned" under certain conditions; (III) The proposal to express "epistemological" considerations in the formal mode of speech.

Von Juhos maintains that each of these points is a thesis or a postulate of physicalism: he speaks of "the physicalist mode of speech which demands the alterability of all statements" (l.c. p. 89; in this passage, elements of each of the three independent points are combined), and he declares the recommendation of the formal mode to be "a special postulate of Physicalism" (l.c. p. 90). For the sake of clarity, it ought to be noticed, that only (I) is a thesis of physicalism, whilst (II) and (III) are fully independent of it.

¹ B. v. Juhos, *Empiricism and Physicalism*; *Analysis*, 2, 81.

² Hempel (1): *On the logical positivists' theory of truth*; *Analysis*, 2, 49.

Indeed, physicalism³ asserts that the language of physics is a universal language of science ; i.e. that "every sentence of any branch of scientific language is equipollent to some sentence of the physical language, and can therefore be translated into the physical language without changing its content."⁴

This thesis obviously entails the special assertion that the statements of psychology can be translated into the physical language.⁵ In the Vienna Circle, this was originally called the *thesis of behaviorism*. At present, this designation is rarely employed, in order to avoid confusing this logical thesis, which deals with the syntax of scientific language, with the psychological statements and the methodological principles of American behaviorism. But as v. Juhos employs the term "behaviorism" in order to denote the syntactical thesis under consideration, it may be suitable to do the same in the following discussion.

So (I) is in fact, a thesis of physicalism. (III) is independent of physicalism : the thesis of behaviorism as well as many other theses of this kind can be formulated both in the material⁶ and in the formal mode. And obviously point (II) is not a consequence of physicalism either ; this is illustrated by the fact that in earlier papers serving to establish the thesis of physicalism (especially in (1)), Carnap started from the assumption that science is based on protocol-statements which need no confirm-

³ See e.g.

Carnap (1) : *The Unity of Science* ; London 1934.

(2) : *Philosophy and Logical Syntax* ; London 1935 (pp. 88-97).

(3) : *Testability and Meaning* ; (will be published in *Philosophy of Science*, 1936).

(4) : *Über die Einheitssprache der Wissenschaft* ; (will be published in the Transactions of the International Congress of Scientific Philosophy, Paris 1935. Publishers, Hermann & Cie, Paris.)

Neurath (1') : *Physicalisme* ; *Scientia* 1931, p. 117.

(1'') : *Physikalismus* ; *Scientia* 1931, p. 297.

(2) : *Physicalism* ; *The Monist* 1931.

(3) : *Empirische Soziologie*, Wien 1931.

(4) : *Soziologie im Physikalismus* ; *Erkenntnis* 2, 393.

Schlick (1) : *De la relation entre les notions psychologiques et les notions physiques* ; *Revue de Synthèse* X, 5 (1935).

⁴ Carnap (2), p. 89. In the meantime, Carnap has developed a certain modification of physicalism (see his papers (3), (4)). For our purposes it is, however, not necessary to refer to this refined form of the physicalistic thesis.

⁵ For a detailed account of this thesis see e.g.

Carnap (5) : *Psychologie in physikalischer Sprache* ; *Erkenntnis* 3, 107.

(6) : *Les concepts psychologiques et les concepts physiques sont-ils fondamentalement différents ?* *Revue de Synthèse* X, 43 (1935).

Hempel (2) : *Analyse logique de la psychologie* ; *Revue de Synthèse* X, 27 (1935). (Here much further literature is indicated).

Neurath (5) : *Einheitswissenschaft und Psychologie*, Wien 1931.

⁶ See e.g. Carnap (7) : *Erwiderung auf die . . . Aufsätze von E. Zilsel und K. Duncker* ; *Erkenntnis* 3, 177 (especially pp. 183-187, pointing out the dangers which arise from employing the material mode in the discussion of the syntactical thesis of behaviorism.)

ation (and are, therefore, not alterable) ; and that Schlick in a recent paper⁷) expressly acknowledges physicalism though rejecting the assertion (II).

On (I). Let us now consider first those of v. Juhos's objections which really concern physicalism, viz. those directed against (syntactical) behaviorism.

(a) In a rough formulation⁸, the thesis of behaviorism (physicalism) asserts, that for any statement speaking of 'feelings,' 'thoughts,' 'acts of will', etc. of a person, there is an equipollent statement which speaks exclusively of the "bodily" behaviour (movements, sounds pronounced, physiological reactions, etc.) of the person in question ; and v. Juhos is right in asserting that, according to behaviorism, the indications of a man about the state of his own feelings (e.g. his saying or writing "I feel pain") have the same logical function for testing a psychological hypothesis (: "This person is in the pain-state") as the pointed, written or spoken indications of a barometer have for testing a physical hypothesis (: "In the interior of this instrument, the air-pressure is now 1 atmosphere").

As a consequence, incorrect indications of the human "pain-indicator" correspond, for behaviorism, to incorrect indications of an air-pressure-indicator or some other instrument. But saying, as v. Juhos does, that for this reason "the physicalist . . . must in cases of the above-mentioned type reproach the instruments in question with a lie or an error" (l.c. p. 87) is evidently running just contrary to the fundamental idea of behaviorism : that attitude would mean applying to physics the point of view of (a rather primitive form of every-day-life) psychology, whilst behaviorism tends to proceed vice versa.

In fact—and this is accentuated by behaviorism—the methodological principles of scientific psychology are the same as those of physics ; and particularly in the case of incorrect indications one blames neither the instrument nor the test-person : one rather tries to *explain* the occurring "deviations," e.g. by showing that certain disturbing factors are working. Formally, such an explanation consists of the establishment of a hypothesis concerning disturbing factors.

(b) In psychology, two particularly important kinds of hypothesis of this type are error-hypotheses and lie-hypotheses.

Now, v. Juhos thinks (l.c. p. 84, ff.) that for explaining

⁷ Schlick (1), p. 14.

⁸ See Hempel (2).

incorrect indications of a man about his own state of feeling, physicalism demands (or at least admits) the introduction of an error-hypothesis ; and he asserts that this "has no meaning for the Empiricist" (l.c. pp. 85/86). It ought to be noticed, however, that *physicalism makes not the least allegation concerning the question of lie and error*, as may be seen from the above formulations and from the papers cited. Therefore, even if v. Juhos were right in asserting that scientific psychology does not admit of error-hypotheses in the considered cases, this would not have consequences for physicalism.

Besides that, it seems to me, that his assertion is not right :

Allowing, or generally forbidding, the introduction of error-hypotheses concerning the results of psychological self-observation ("introspection") is, I think, a question of syntactical convention, which cannot be true or false, but only more or less practical. In the case of allowing, the convention mainly consists of specifying how to test such an error-hypothesis. Hereby, the error-hypotheses are *given* a "meaning."

As to actual scientific psychology, I think that it would *not* generally exclude error-hypotheses concerning the results of self-observation. E.g. Any psychologist would admit the possibility (and even the frequent occurrence) of error when a man states through self-observation the motives of his actions ; e.g. when he says : "I change my political party out of conviction ; I am sure that I have not got the least desire for material advantages." (In cases of this type, psychoanalysis, e.g., furnishes us with certain testing-methods.) And it seems to me that there is only a difference of degree between this case and that of a person saying "I feel pain" or "I see blue."

And a more general point of view : if lie-hypotheses were not admitted in the latter case, a sentence of the form "When pronouncing the statement S, the person A committed an error" would be meaningful or meaningless (not false !)—i.e. it would belong or not belong to scientific language—according to the words occurring in S ; and this would be so even if S itself ("I

* v. Juhos says that "while formulating a proposition such as 'I feel pain', I know already whether I have made a true or a false statement" (l.c.p. 84). I think, that with equal justice, one could say : "When formulating a statement about the real motives of my action, I know already if I am right or wrong."

In quite a similar connexion, v. Juhos asserts that, concerning the falseness of contradictory statements, an error is impossible, and that somebody who pretends that a certain contradiction is true, must purposely make a false statement (l.c. pp. 89/90). From the experience I acquired when teaching mathematics at school, I think that on this point v. Juhos is certainly not right.

feel pain ") is a formally correct and meaningful statement of the language under consideration. This would obviously be very little expedient. Therefore, I think that scientific psychology would not exclude once and for all error-hypotheses concerning statements like "I see blue."

However, this question cannot be discussed here in more detail, because it is independent of the problems of physicalism.

(c) Von Juhos further objects to physicalism leading to such "absurd" "consequences" (l.c. p. 85) as the assertion that people who do not know the pain-state-criteria of contemporary psycho-physiology, do not mean anything when speaking about pains. This *is* absurd, indeed, but it *is not* a consequence of physicalism. Physicalism only refers to the fact that all the methods employed in every-day life and by science for testing statements about feelings, thoughts, and such like of a man consist in examining his behaviour. In every-day life, one refers principally to such characteristics of behaviour as play of features, gestures, blushing, crying, etc. Scientific psychology, which has at its disposal many more empirical laws than every-day life, knows that those characteristics are empirically connected with other ones (e.g. certain nervous reactions) which, consequently, may likewise serve for testing the statement in question. But the introduction of new testing-methods does not render the older ones "meaningless" (though they may turn out to be less exact)—just as in physics the introduction of a new (e.g. electrical) method of testing a temperature-indication does not imply the rejection of the former methods (such as using a mercury-thermometer or even mere judging by touching the body in question) as "meaningless."

(d) Finally, v. Juhos gives an example (l.c. p. 88/89) meant to illustrate the possibility of testing certain psychological statements directly, i.e. without referring to the "bodily" behaviour of the person in question. Without discussing this general question here (it had better be put into the formal mode before being examined), I want to remark, that the example given by v. Juhos seems to me not to stand its ground. For how can the blind man (B) first know that his colour-experiences occur in regular connexion with those of the seeing man (S), and that they may therefore serve him in future for "directly" controlling S's colour-statements? At first, S must tell him one or several times, when he, S, is seeing blue. And B, when noticing that

each time he himself has likewise a colour-experience, may state a general law : Each time I have a colour-experience, S has one, too. And this law may serve him for further controlling S. But B would *not* be able to establish the law *without* knowing first the indications of S, which are at the same time a *part of S's bodily behaviour*.

Concerning (II). (e) It first has to be noticed that the conception of certain statements as unalterable or the stipulation that no statement of empirical science has to be maintained at all costs, is a matter of convention between scientists ; this has been accentuated particularly by Carnap.¹⁰

On the other hand, one may ask, which of the two possible conventions is in better congruence with the actual methodological attitude of our empirical science. As to this question, first Neurath¹¹ and Popper,¹² later joined by Carnap in their view, have insisted upon the alterability of *any* statement in empirical science ; but they do *not* pretend that " we are allowed to alter the propositions obtained by observation as it pleases us " (v. Juhos, l.c. p. 83) ; on the contrary : a statement which is once acknowledged (and in particular an observation-statement) can only be abandoned if it is incompatible with another statement which is confirmed by observation in a very high degree.¹³

(f) As to the self-observation statements, there exists at least the possibility of introducing a lie-hypothesis, and therefore it is possible, that even a statement of this kind should be abandoned. v. Juhos himself admits : " In stating ' I feel pain ' I may have said something wrong . . . because I wanted to lie " (l.c. p. 84). And this evidently invalidates his assertion that statements such as " I feel pain " are unalterable, for the possibility of error concerning them does not exist " (p. 84). No : such a statement might be altered—at least by adopting a lie-hypothesis. As to the question of this being the only possibility, see (b).

Moreover I wish to emphasize that when discussing the

¹⁰ See Carnap (8) : *Über Protokollsätze* ; *Erkenntnis* 3, 215 ; and Carnap (3).

¹¹ See e.g. Neurath (6) : *Protokollsätze* ; *Erkenntnis* 3, 204.

(7) : *Radikaler Physikalismus und "wirkliche Welt"* ; *Erkenntnis* 4, 346.

(8) : *Pseudorationalismus der Falsifikation* ; *Erkenntnis* 5, 353.

(Here, Neurath expressly accentuates that observation-statements—for which he proposes a certain common form—possess a "greater stability" (p. 362) than many other statements in empirical science : they need not be altered so often as the latter ones.)

¹² See K. Popper (1) : *Logik der Forschung*, Wien 1935. This book contains a detailed theory of the principles of testing in science and in particular an important and original account of the alterability of any empirical statement.

¹³ For more detail, see particularly Popper (1).

possibilities of lie and error in this connexion, v. Juhos seems to start from a misunderstanding similar to that concerning physicalism (see (b)): the adherents of the alterability of any scientific statement do *not* pretend that the alteration of the considered psychological statements must always be carried out by assuming an error; the distinction between lie and error is quite independent of the problem in question.

On (III). (g) Finally, v. Juhos develops a very interesting objection against the proposal of employing the formal mode of speech¹⁴ in epistemological discussion (l.c. pp. 90-92). He refers to a formulation given by me: "The system of protocol-statements which we call true . . . may only be characterized by the historical fact, that it is . . . actually adopted . . . by the scientists of our culture circle." Dr. v. Juhos translates this into the formal mode as follows: "the system of protocol-statements that we call true is characterized by the quality that certain protocol-statements belong to it, which assert that this very system of statements is acknowledged as true by the scientists of our cultural circle" (p. 91); from this remark, v. Juhos deduces the "objection . . . that an infinite number of systems of protocol-statements which are not to be contradicted might be quoted, all of which contain those particular statements, which characterize as true the system of protocol-statements of our science, but for the rest are incompatible with this system. And all these imaginable systems have to be considered true exactly in the same sense as our true science" (l.c. p. 91).

However, the translation given by v. Juhos is not adequate. The term "*historical fact*" serves to express a reference to that which is *acknowledged* as factual *by our science*. For the translation into the formal mode, we have to replace "It is a fact, that . . ." by: "The statement . . . is sufficiently confirmed by the protocol-statements adopted in our science." So, the following translation of the cited passage results: "The following statement is sufficiently confirmed by the protocol-statements adopted *in our science*: 'Amongst the numerous imaginable consistent sets of protocol-statements, there is in practice exactly one which is adopted by the vast majority of instructed scientific observers; at the same time, it is just this set which we generally call true'." The whole is by no means a contradictory pro-

¹⁴ See e.g. Carnap (1), p. 37 ff.; (2), p. 68 ff.

(10): *On the character of philosophic problems; Philosophy of Science I*, 5 (1934).

position, nor can one deduce from it the consequences indicated by v. Juhos.

In the end, v. Juhos's objection furnishes us with a new example in favour of the proposal to use the formal mode of speech in logical discussion ; for, as we saw, my earlier material formulation caused a misunderstanding which is now, I hope, cleared up.

Brussels, December 1935.

PRE-EXISTENCE AND FREEWILL

By HELEN M. SMITH

MR. PRICE is of the opinion that the chapter on Freewill in Mr. Wisdom's *Problems of Mind and Matter* may seem to some readers "the most exciting in the book", and philosophers at the Joint Session in July spoke of it with special interest. Further analysis of the arguments contained in the chapter seems to me to be not inapposite, for I cannot but wonder if there is not some confusion as to what exactly they amount to in the minds of those they excite.

What is exciting is the idea that Mr. Wisdom has produced arguments in favour of pre-existence. "Pre-existence" he says "follows from our considerations in this Part of the book" (p. 130). There are two relevant arguments, but the one to which most importance is attached is the "argument from blame", and it is with it I am concerned. Mr. Wisdom begins by considering in what sense freewill is "curtailed" by blame. A man is not to blame for a murder unless he acted voluntarily and could have held his hand had he decided to do so : he is not to blame if his volition has been "completely determined" by factors external to himself : he is not to blame if he has been at the mercy of the gods, of heredity, or of the material universe. Thus blame entails that the culprit was free in the sense, Mr. Wisdom avers (pp. 116-8), that nothing and no-one was the complete cause of the volition which brought about the crime, and this "however far back we go". But if this is what blame entails, and if—as the law of causation asserts—every event is completely determined, then the culprit's own will, "however far back we go", must have been a cause-factor determining the

volition for which he is now blameworthy. Therefore the culprit must have pre-existed.

Now I think that the language of the chapter may have suggested to the minds of some readers an "argument from blame" which could be formulated as follows. We sometimes blame truly; blame implies either indeterminism or pre-existence; therefore either indeterminism is true or we pre-exist. But indeterminism is not true; therefore we pre-exist. Such an argument is invalid. It does not satisfy the epistemic conditions of inference; and though it is one of the peculiarities of the freewill controversy that some of those taking part in it have been proud to use invalid arguments, I do not think Mr. Wisdom is one of them. On his own showing we are not in fact blameworthy unless we pre-existed. Therefore to *know* we are blameworthy we must also know that we pre-existed, and cannot assert the premiss unless we already *know* the conclusion. Moreover it seems evident to me that we have no knowledge of pre-existence. Certainly we judge men to be blameworthy without either knowing or asking if they have pre-existed. Therefore if judgements of blame imply pre-existence it goes to show, not that we have pre-existed, but that we are never justified in attributing blame.

This is indeed an exciting conclusion, but it is not Mr. Wisdom's. The existential import of his language may be regarded as incidental to the real purpose of the chapter which is to attempt a solution of the Freewill and Determinist antinomy by careful attention to what the protagonists of these doctrines respectively contend (pp. 112-3). What Mr. Wisdom has done is to offer and defend an analysis of what is meant by freewill which, if we accept pre-existence, is compatible with the law of causation. From this point of view pre-existence may be regarded, not as the conclusion of an argument, but as a mediating hypothesis. If the belief in pre-existence is accepted, suggests Mr. Wisdom, the ordinary man's belief in freedom of choice and the belief that every event has a cause can be reconciled. That is to say, the ordinary man can hold these beliefs without contradiction provided he accepts also the belief in pre-existence that their conjunction entails (whether or not the beliefs are true is another point). But he can do so, I must insist, only if he accepts Mr. Wisdom's analysis of freewill. I am not sure whether this analysis represents Mr. Wisdom's

considered opinion, or whether it is part of a logical exercise in speculation. In any case it is the essential and interesting point on which the argument depends. On this analysis to say "You were free to get up five minutes earlier this morning" is to say something about the whole of past time; for it is to say something that entails that "however far back we go in setting out the causes of your act, we shall never come to a time at which a set of purely external circumstances, not involving you and your will, formed a complete cause of your act" (p. 118).

Now it must be granted that with this analysis of freewill determinism is not incompatible, provided we accept pre-existence. The law of causation asserts that however far back we go we shall always find "a set of circumstances that formed a complete cause" of your act. Therefore, if you have freewill, "you and your will" must always have been involved. The question is how would these beliefs affect the ordinary man's use of the term 'blame' as applied to wrong acts done voluntarily? Would he blame as heartily if he were told that a voluntary act is one that is determined by the psychology of the agent in the circumstances in which he finds himself *from all time*? Would he not feel like Kant that this kind of freedom is too like that of a "free projectile"? It seems clear to me that there is at least one sense of the term which he would hesitate to apply, namely, the very sense with which Mr. Wisdom is here concerned. It is because this is so that the deterministic psychology of the Freudians has so subtly affected the moral judgements of our generation. There are certainly some senses of 'blame' that are still applicable on a deterministic psychology, though they are senses with which pre-existence has nothing to do. To blame a man may be to say that his motives are bad, or that his conduct justifies punishment. The word is no doubt used in many different senses according to circumstances and temperament. But the sense which would justify indignation as distinct from pity, and on which retributive punishment and the ordinary man's sense of obligation is based, is not one which would be applied by the ordinary man if he could be convinced that the wrong action was psychologically determined. He would say of the culprit "he can't help it." Thus, unless we are prepared to reject the law of causation, either this sense of blame is inapplicable or Mr. Wisdom's analysis of freewill is wrong.

By working out the implications of his analysis Mr. Wisdom

shows that it is incompatible with some of the ordinary man's judgements of blame. It seems to me that the sense of freedom that these judgements require is one which has nothing to do with past time, but only with the choice of the moment. All past events might have been completely determined by factors external to the self provided only that the action for which we are blamed is done voluntarily. Freewill might have been evolved or been received the hour before. But however that may be the alternative is clear. Either we must recognise that judgements of blame in the sense under discussion are inapplicable or we require a sense of freedom other than Mr. Wisdom's. In reconciling belief in this other sense of freedom with belief in the law of causation there seems no reason to think that the notion of pre-existence will be helpful.

August 1935.

THE ANALYSIS OF SENSE-DATA: A REPLY TO MR. PAUL

By J. O. WISDOM

IN *Analysis* No. 9 I wrote a philosophic mole-hill of two pages on "The Analysis of Sense-Data." Of this Mr. G. A. Paul, in *Analysis* Nos. 11 and 12, made a philosophic mountain of eight pages. May I add a cairn?

Mr. Paul finds it difficult to understand attempts made to analyse sense-data and says that my attempt is no exception. It was a source of great satisfaction to me, therefore, to discover that Mr. Paul had understood my article extremely well, I cannot accuse him of lack of insight or unfairness *anywhere*—how often are authors so graciously treated? One infers that, if my analysis was such nonsense as Mr. Paul implies, he was rash to penetrate its "meaning." I do not, however, regard Mr. Paul's criticism as entirely satisfactory; and I wish therefore to reply on behalf of those whose spokesman I have been appointed by Mr. Paul. (I may say I was not aware of acting in this capacity when writing my article, nor am I aware of it now.)

(1) Mr. Paul objects that I do not give instances of the kind of analysis I was seeking when analysing sense-data. I doubt if there are any *other* instances of exactly the same sort; but, if so, that is not my fault, and uniqueness would not prevent my analysis

from being perfectly respectable. It seems to me that all analysis arises in this way : a statement appears to have certain implications ; something is *felt* to be *queer* about these implications, or else there seems no way of choosing between incompatible implications ; accordingly we think that the question whether the statement *has* queer implications or incompatible ones depends upon what *meaning* the statement has ; and so we set about analysing the statement. In other words we have to define statements in such a way that no paradoxes arise. This then is my "*criterion*" by which we shall know whether the analysis has been found." To this I shall return in (4).

(2) Mr. Paul should use arrows instead of inverted comma for branding "barbarous" philosophical phrases. Not all the barbarous phrases were mine ; some were "merely philosophical phrases which" he "felt compelled to use." He does not, however, explain why he uses them if he thinks so ill of them. Presumably he can only talk to me in my language. Now why shouldn't we use such "barbarous" phrases as "must," "cannot," "logically impossible" ? Are they not in the dictionary as part of our language ? Very likely not, because the dictionary compositors do not consider them barbarous enough to have inverted commas. But what does Mr. Paul mean by "barbarous" ? Presumably that the use to which they are put by philosophers offends against the canons of logical analysis. But logical analysis must not be so narrow. If barbarous phrases will enable me to say shortly what can only be said at length in the language of analysis, then I will certainly use them : though it is safer if one keeps an eye open to see that unbarbarous translations could if necessary be at any moment effected. This is a regular procedure in mathematics. The mathematician does not always invoke the rigour of mathematical analysis. He does so only if he cannot see what difference a rigorous treatment would make. In logical analysis, too, Professor Carnap advocates the need for the formal mode of speech, but he does not assert—quite the reverse—that in practice it is always necessary to use the formal mode.¹ Further, in my paper, I did not attempt to give definitions of barbarous phrases, because I thought the definitions too well known and because I could not have all the pages of *Analysis* for myself.

(3) I gave as the analysis of sense-data : *Herenow is character-*

¹ See *The Unity of Science*, pp. 83-4, and *Philosophy and Logical Syntax*, p. 80.

ised by a colour and by a shape. Mr. Paul points out an ambiguity in "is characterised by," and asserts quite correctly that I have not used it in the well known sense in which a thing is ordinarily said to have characters. And indeed I cannot *define* the phrase, for like "Herenow" it is indefinable; but that is not to say its usage cannot be explained. Is Mr. Paul confusing the psychological order in which we know things with the logical order by which we arrange definitions in a series of decreasing levels from definables to indefinables? Or perhaps Mr. Paul thinks me guilty of a logical circle (instead of a psychological one as I have just implied) when he writes that to say that a Herenow has characters is to say (at least) that a time has characters, and that to say that a time has characters is probably to say that something has some character (in some ordinary sense) *throughout* that time. (From this Mr. Paul infers that "is characterised by," as used by me, only has some ordinary sense, and if so is useless for the analysis I was seeking.) However this translation seems to me plainly false. For "*throughout that time*" means "*throughout the time element of a Herenow,*" which is not clock-time but consciously apprehended or sensed time. Now whatever can have some character *throughout* sensed time, it is certainly not physical objects. Sense-data could; but for this case "having some character" has never been defined; and is in fact indefinable unless my analysis be counted a definition of it. Accordingly Mr. Paul has not reduced my analysis to one involving "has some character" *in some ordinary sense*.

(4) Lastly to deal with Mr. Paul's main contention. This is that my analysis succeeds only in giving information as to how I wish to use certain words, e.g. "sense-datum," "Herenow," and "is characterised by." That is to say my paper merely explained my own special terminology. This is a very potent way of dealing with a question. It springs from an attitude, which I feel sure Mr. Paul adopts, and which I certainly do, that *there are no "philosophic" problems* (in the "barbarous" sense of "philosophic"), and that if a discovery is claimed, it will be found to involve either nonsense or a definition; for a statement is either synthetic or analytic, and a synthetic statement depends upon experience while a philosophic discovery claims to be more than analytic without being dependent on experience. I concede this to Mr. Paul. And I concede that it is scarcely an injustice to say that my paper was a statement of terminology, though it

is a little misleading to say so. I meant by my exposition to imply that I considered the terminology in question *important* i.e. a better one than certain others. I did not think this worth while mentioning because it seemed obvious—it would be scarcely human to consider the main point of one's paper unimportant. But in what way could I consider my terminology important? In this, that it seems to avoid certain paradoxes which appear to arise in connexion with unperceived sensibilia or the specious present. The definitions of " $\sqrt{2}$ " (Dedekind), "a point" (Professor Whitehead), or of "a proposition" (Professor Broad) are merely statements of terminology; but they are "correct" if irrational numbers, points, and propositions as so defined do all the things we want them to do *without* giving rise to paradoxes. It was this "correctness" I was aiming at by my analysis of sense-data.

London, December, 1935.

MORE ABOUT "ABOUT"¹

By ROSS THALHEIMER

IN an article entitled "'About,'" which appeared in the first number of the first volume of *Analysis*, the writer, Mr. G. Ryle, wanted "to distinguish several senses in which a sentence is said to be 'about' something, or alternatively, in which the author of a sentence is said to be talking 'about' something." In the present article I wish to make the following comments upon Mr. Ryle's remarks.

(1) Mr. Ryle says: "'The sentence S is about Q' often means 'in the sentence S 'Q' is the grammatical subject or nominative to the verb (or main verb)'"'. Comments: (a) I doubt very much if "The sentence S is about Q" *does* often mean this. (b) I think, however, it *does* often mean something *similar* to this, viz., "Q is what is denoted by the grammatical subject or nominative to the verb (or main verb)." And (c) I think it is quite possible that Mr. Ryle came to assert the proposition of whose truth I have indicated I am very doubtful partly because of his failure to *distinguish* these two meanings,

¹Mr. Thalheimer writes: 'there has been organized at Johns Hopkins University this year a philosophy club which is devoting itself to the discussion solely of articles appearing in your Journal. The substance of the paper I am herewith enclosing was . . . presented at the first meeting of this Club.' Editor of *Analysis*.

The following sentence may be helpful in making the distinction between these two meanings more clear to the reader. To say "in the sentence S 'Q' is the grammatical subject or nominative to the verb (or main verb)" is to say something which is merely about the grammatical structure of S at least in some sense of "about" in which to say "Q is what is denoted by the grammatical subject or nominative to the verb (or main verb)" is *not* to say something which is merely about the grammatical structure of S. Incidentally, this sense of "about" is (or these senses of "about" are) not identical with any of the senses of "about" enumerated by Mr. Ryle in his paper.

(2) Mr. Ryle says: "'the sentence S is about Q' often means 'the sentence S contains "Q" and "Q" is a noun or pronoun or a phrase equivalent to a noun or pronoun occurring in no matter what grammatical position in the sentence'." Comments: (a) I doubt very much if "The sentence S is about Q" *does* often mean this. (b) I think, however, it *does* often mean something *similar* to this, viz., "Q is what is denoted by a noun or pronoun or a phrase equivalent to a noun or pronoun occurring in no matter what grammatical position in the sentence." And (c) I think it is quite possible that Mr. Ryle came to assert the proposition of whose truth I have just indicated I am very doubtful partly because of his failure to *distinguish* these two meanings.

(3) Each of the two possible confusions to which I have referred above may be very roughly described as a possible confusion between a symbol and what that symbol denotes. I think it is equally likely that Mr. Ryle is guilty of confusions of this roughly described general type in setting forth the other two senses of "about" which he distinguishes, if not in giving the definitions themselves, at least in offering the accompanying explanations. He says, e.g., "Sometimes, a sentence contains two or more nouns or virtual nouns but of them one is naturally thought of as that which is being talked 'about' (in a third sense)." But I do not think it will be desirable to pursue this point further here.

(4) Mr. Ryle says that he "cannot explain the existence of" the "presumption, often unwarranted, that if S is about(l) Q, then S is about(r) Q," that is to say, that if S is "about" Q in some one or more of the first three senses which are distinguished, it is "about" Q in the fourth sense. Comment: I do not know if Mr. Ryle wished this statement to be construed literally,

or simply as an expression of a sort of impatience with those who make this presumption. But if, as I think, the former is the case, it may not be inappropriate to suggest that Mr. Ryle might, without inconsistency, have explained the existence of the presumption as resulting from the fact that it very frequently happens that where it is true to say that S is about(l) Q, it *is* also true to say that S is about(r) Q.¹ (I should like to add parenthetically that had Mr. Ryle stated explicitly that "S is about(r) Q" entails the existence, in some sense, of Q—a proposition which is, I take it, *implicit* in what he says—some of his remarks upon this fourth sense of "about" would have gained somewhat in clarity.)

(5) Mr. Ryle concludes his article with the statement: "I do not suppose that I have exhausted the senses in which a sentence is 'about' something." Comment: I do not know if Mr. Ryle wished this statement to be construed as asserting that he is in *doubt* as to the completeness of his enumeration, or as asserting merely that he does *not assume* that his enumeration is complete. Certainly (i.e., *I am certain*), however, if the former is the case, that doubt is fully justified: the enumeration is not complete. In sections 1—2 of this article I have defined two senses not mentioned by Mr. Ryle in which a sentence is "about" something, and pointed to at least a third. And still others may be distinguished.

Thus, consider, e.g., the second of the senses enumerated by Mr. Ryle and the sense (in my opinion, considerably more important) which I have said Mr. Ryle may have confused with it. In both of these senses, there is a restriction to nouns, pronouns, and phrases equivalent to nouns or pronouns. But if we remove this restriction, we arrive at one or two additional senses, in which a sentence is "about" something. It is in such an extended sense of "about" that the word is, I think, frequently used when it is said, e.g., that "This is to the right of that" is about this, that, and to-the-right-of, or that "John admires George" is about John, George, and admiring. This use of the word "about" is, indeed, very common among philosophers. Yet if we retain Mr. Ryle's restriction to nouns, pronouns, and their equivalents, no place can be found for it in our list.

¹ To avoid complicating this sentence, I am here ignoring the point that I doubt very much if "about" *is* often used in some of the senses Mr. Ryle says it is.

